at this theatre for about a year and a half without any accidents, and has proved to possess many advantages over gas as applied to the illumination of buildings of this description. Not the least amongst these are the total absence of heat and vitiated air in the house, and the length of time during which the decorations will retain their freshness and colour instead of becoming quickly faded and tarnished, as would be the case were the old system of gas adopted.

ON THE NATURE OF INHIBITION, AND THE ACTION OF DRUGS UPON IT

BY inhibition we mean the arrest of the functions of a structure or organ, by the action upon it of another, while its power to execute those functions is still retained, and can be manifested as soon as the restraining power is removed.

It is thus distinguished from paralysis, in which the function is abolished, and not merely restrained.

Inhibition is one of the most perplexing problems in physiology, and we have at present no satisfactory hypothesis regarding it. It plays, however, such a very important part in pharmacology, that we cannot pass it over; and as it is through the action of drugs upon the various functions of the body that we have already arrived at a knowledge of inhibitory actions, which would otherwise have been impossible—as, in fact, pharmacology has here quite outstripped physiology—we are obliged to enter into some hypothetical considerations, in order to be able to form some kind of idea regarding the mode of action of many drugs.

Hypotheses serve as "pegs on which to hang facts," and by their aid the isolated facts which few memories could carry may be arranged, and their relation to each more readily perceived. A hypothesis serves also as a guide for further experiments, by which it may be either disproved or supported. Should facts be against it, so much the worse for the hypothesis; it must be discarded, and another tried in its place; but if facts agree with it we obtain a means of predicting phenomena, and make another step in knowledge. Like other useful things, hypotheses are not without danger, and sometimes do harm by satisfying people and stopping further inquiry. Thus Sultzer noticed the peculiar taste produced by the contact of two dissimilar metals with each other and with the tongue forty years before Galvani; but at that time the doctrine of vibrations was employed to explain all natural phenomena, and he concluded that some peculiar vibration occurred from the contact of the metals, which produced the peculiar sensation on the tongue. All the world were satisfied with the explanation, and thus a prominent fact slept in obscurity from the time of Sultzer to that of Galvani, no further attempts being made to determine the nature of the vibrations or the laws which governed them.[1] Yet in their proper place hypotheses are most useful, and but for the hypothesis that light, heat, and sound are due to waves, our knowledge of their phenomena would be much less than it is.

The cases of inhibition, as we may term them, which we meet with in the study of physics, are the production of complete silence by the interference of two sounds and of darkness by the interference of two rays of light.

When two sounds or two rays of light are combined, so that the crests of the waves of which they consist coincide, the sound becomes louder and the light brighter. If they are thrown together, so that the crests of the waves in the one sound or ray coincide with the sinuses or hollows of the other, they completely counteract each other, and silence or darkness is produced.

When the waves are of different rhythms, the crests and hollows of the two sounds or rays, which at one time coincide, will gradually interfere, and again gradually coincide, so that rhythmical alternations of loud sound and silence, of bright light and darkness, are produced.

A good example of interference or physical inhibition, and one that affords an illustration well suited to our purpose, is that of Newton's rings. When a lens of small curvature is placed on a plane surface of glass, a series of rings is observed, starting from the centre of the lens and passing concentrically outwards. If monochromatic light is used, such as pure yellow light, pure red light, &c., these rings are alternately bright and dark; but if white light is used, they appear as a number of circular bands of different rainbow colours. The cause of these rings is, that though the surface of the lens appears to the eye to be in contact with the plate of glass over a considerable area, it is not really so; a very fine film of air of varying thickness being interposed between them.

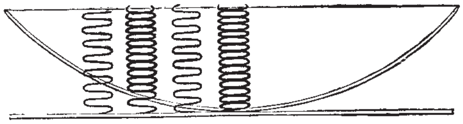

FIG. 1.—A very diagrammatic representation of interference in Newton's rings.

When a ray of light passes through the lens on to the glass, part of it is reflected back from the lower surface of the lens, and part of it from the upper surface of the glass plate. Between those two points there is a very minute film of air: *one ray has therefore to travel somewhat further than the other.* The distance which it has to travel is only through the extremely thin layer of air lying between the surface of the lens and the glass and back again; but this distance at some places is just sufficient to throw the waves in the one beam *half a wave-length* behind those in the other, and to produce darkness by their interference.

As we recede from the point of most complete contact between the lens and the glass, the thickness of air increases, the ray has somewhat further to travel, and the distance is then just sufficient to throw it *a whole wave-length* behind the other ray; no interference is produced, and we get a ring of bright light.

Further outwards the increased thickness of the film of air is again sufficient to throw one ray *a wave-length and a half* behind its fellow; interference is again produced, and darkness is the result.

With rays, then, of one colour, or of one wave-length, we get alternately light and darkness by interference.

But it is evident that the extra distance which the waves have to travel in order to produce interference will not be the same for long and short waves; and thus it is found that when white light, which contains rays of different wave-lengths, is used, the rings, instead of being alternately light and dark, are coloured.

The very distance which was sufficient to throw the red rays half a wave-length behind the other, and to produce interference, will throw, let us say, the violet rays a whole wave-length behind, and thus there will be no interference and *vice versâ;* the distance which causes interference of the violet rays does not cause interference of the red, and so on with other colours.

Thus the spaces which would have been perfectly dark when rays of pure red or pure violet, or more correctly ultra-violet, were used, would be filled up by the other if used together, and when white light is used, the various waves interfere at different places, and so we get a series of rainbow colours.

The extra distance which one beam has to travel in order to produce interference with another is *not absolute*, but relative to the wave-length. This relation differs for different wave-lengths, and therefore if the relative distances remain constant, the effect of the beams on each other will vary if their wave-lengths be changed.

It is obvious that if both the wave-lengths and the distances they have to travel remain the same, the effect of the beams on each other will be altered by any change in their rate of travel such as would be effected by altering the media through which they pass.

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[1] Ree's Cyclopedia. Article "Galvanism."

This is a most important point in regard to the hypothesis of the causation of inhibition by interference of vibrations in the nervous system. It may therefore be useful to illustrate this further, and probably it could not be done better than by using, with a little modification, the example given by Sir J. Herschel in his article on Light in the Encyclopædia Metropolitana. "Let R be a reservoir of water, from which the channels A and B proceed, to join each other at P; they are supposed to be equal in every respect except that B is longer than A. If a wave from the reservoir enters the openings of A and B at the same time and travels at the same rate along them, the wave which passes through A will reach P sooner than the one which passes through B, so that the water at that point will be agitated by two waves in succession. But let the original cause of undulation be continually repeated so as to produce an indefinite series of equal and similar waves. Then if the difference of lengths of the two canals A and B be just equal to half the interval between the summits of two consecutive waves, it is evident that when the summit of any wave propagated along A has reached the point of intersection P, the depression between two consecutive summits (viz., that corresponding to the wave propagated along A and that of the wave immediately preceding it) will arrive at the intersection P by the course B. Thus in virtue of the wave along A the water will be raised as much above its natural level as it will be depressed below it by that along B. Its level will therefore be unchanged. Now as the wave propagated along A passes the intersection P, it subsides from its maximum by precisely the same gradations as that along B, passing it with equal velocity, rises from its minimum, so that the level will be preserved at the point of intersection P undisturbed so long as the original cause of undulation continues to act regularly.[1] So soon as it ceases, however, the last half-wave which runs along B will have no corresponding portion of a wave along A to interfere with, and will therefore create a single fluctuation at the point of concourse P."

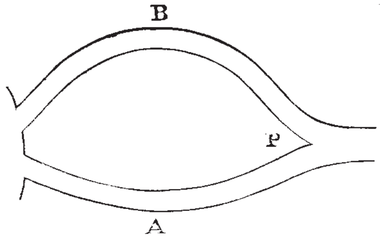

FIG. 2.—Diagram to illustrate Sir J. Herschel's observations on interference. Adapted from his article on "Absorption of Light," Phil. Mag. 1883, p. 405.

It is obvious that if everything else remains the same, the effect which the waves have upon each other at P will be altered if the rate at which they travel is increased or diminished.

The more the speed is increased the less effect comparatively will the greater length of B have in retarding the wave which flows along it, so that its crest will no longer coincide with the trough or sinus of the waves in A, but will, on the contrary, coincide more nearly with the crest of one of the waves in A.

The more the speed is diminished, the more will the wave in B lag behind that in A, so that its crest, instead of coinciding with the trough between two crests of the waves from A will gradually come to coincide with the crest succeeding the trough, and thus double its magnitude instead of destroying it.

We see, then, that under the conditions we have supposed either increase or diminution in the rapidity of their transmission may convert the interference of waves into more or less complete coincidence, and the effect of the two waves may thus be doubled instead of neutralised by their superposition.

The alteration which is produced in the mutual effect of two waves by increase or diminution of their rate of transmission along channels of constant length supplies us I think with a test by which we may ascertain the truth of the hypothesis that inhibitory phenomena in the animal body are due to interference. For if it be true we ought to find that a nerve which produces inhibitory phenomena when excited under normal conditions will gradually lose this power when the rate of transmission along it is increased or diminished, as, for example, by the influence of heat or cold, and will gradually acquire an exactly contrary or stimulating action. This, I think, is shown to be the case by our experimental data so far as they go.

Several authors have pointed out the analogy between inhibitory phenomena in the animal body and the effects of interference of waves of light or sound. This has been done with special precision by Bernard[1] and Romanes.[2] The tendency to do away with the idea of distinct inhibitory centres is gradually spreading, but hitherto no attempt has been made to bring all the phenomena of inhibition under one general rule or to explain the mode in which they are affected by the action of drugs. The object of the present paper is to gather together some instances of inhibition which we find in the body, and to see whether by the theory of interference it is not possible to explain both the curiously perplexing exceptions which we meet with in physiological experiments, and the still more perplexing action of drugs on inhibitory phenomena.

One of the most striking examples of reflex action and of inhibition, is the effect of a slight touch or touches, and of firm pressure upon the palms of the hands, the soles of the feet, or the axillæ, and in some persons also the knees. In many persons a very slight touch or succession of touches upon these parts is sufficient to throw first the respiratory muscles, and then the whole body into violent convulsions. Indeed, it is stated that during the persecution of the Albigenses by Simon de Montfort, several people were tortured to death by tickling the soles of their feet with a feather. The stimulus here applied, and the consequences it produces, appear to be out of all proportion to one another; the stimulus being almost infinitesimal, and the consequences enormous.

In the case of Newton's rings it might be possible with much trouble to throw a different beam into such a condition that it would interfere with one of the beams in the rings and produce darkness, but in the rings a similar effect is produced in a very much simpler way by alteration of part of the same beam. A similar occurrence is to be observed in the inhibition of the reflex action on tickling.

By a very powerful effort of the will we may completely arrest the reflex movement which would otherwise occur, and allow the limb to remain perfectly passive. But the same effect is produced in a much simpler way by applying a firm pressure instead of a slight touch. The firm pressure neutralises the effect of the touch in regard to motion, and not only are no reflex convulsive actions produced, but no tendency whatever to them is felt.

But while the pressure has neutralised the tendency to motion, and has altered the character of the sensation, it has not neutralised sensation. On the contrary, it has rendered it more definite, so that one can distinguish with much greater certainty the particular point of the surface which has been touched. Increased pressure has thus inhibited motion but increased sensation.

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[1] This actually happens in the harbour of Batsha, into which the waves pass from the open sea through two channels of unequal length.

[1] Bernard, La Chaleur Animale, Paris, 1876, p. 371.

[2] Romanes, Phil. Trans. 1877, p. 730.

In a paper on "Inhibition, peripheral and central," which I wrote in the West Riding Asylum Reports in 1874, I tried to explain these phenomena in the following manner: "It appears to me to be in all probability due to there being two sets of ganglia in the cord itself, one motor and one inhibitory. The motor is more readily excited than the inhibitory, and causes violent movements, which the inhibitory centres of the brain cannot restrain without the greatest difficulty, though they are readily controlled by the inhibitory ganglia in the spinal cord. A slight titillation excites the motor, but not the inhibitory spinal ganglia; a stronger pressure stimulates the inhibitory centres also, and thus arrests the movements without any action being required on the part of the inhibitory centres in the brain. We may try to explain this, by supposing that there are two distinct sets of nerves proceeding from the skin to the cord, one of them having the power to excite inhibitory, and the other to excite motor centres. Further, we must suppose that these sets of fibres are endued with different degrees of excitability, the motorial ones being stimulated by a slight touch, but the inhibitory ones only by a stronger impression.

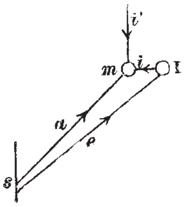

FIG. 3.

"This is represented in Fig. 3, where s is the skin; a, the fibres proceeding from it to the motor ganglion, m and e, those going to the inhibitory ganglion, I; i is the fibre by which I arrests the action of m, and i that by which the brain exerts a similar action. The different fibres by which m acts on the muscles have not been introduced into the diagram.

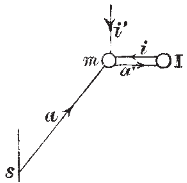

FIG. 4.

"This hypothesis, however, is a very clumsy one, and we explain the facts quite as well by supposing that there is only one set of afferent nerves (a, Fig. 4) from the skin to the cord, which transmit a slight impression only to the motor ganglia, m, but convey a stronger one along a' to the inhibitory ganglia I, also, which then react through i upon the motor ones. This latter supposition renders intelligible the fact that it is only when something is drawn quickly and lightly across the skin, so as to make a slight and transient impression on the ends of many sensory nerves, that tickling is felt. If the pressure on the skin is heavier, or if the motion over it is slow, the effect is quite different, and this is just what we might expect if a short and slight impression travels only to the motor ganglia, and a stronger or more lasting one goes to the inhibitory beyond them."

These diagrams themselves are suggestive of interference; but I did not in that paper say anything regarding it, contenting myself only with the term inhibition. One reason that prevented me from considering inhibition in animals as corresponding closely to the interference of light, was that the rapidity of transmission of nervous impulses was differently given by different observers, and indeed, according to Munk, it varies along the course of the same nerve.[1]

Unless the rate of transmission of impulses is constant, one cannot expect interference to produce inhibition. But in his observations on Medusæ, Mr. Romanes found that when the circumference of the bell in a medusa was cut into a long spiral strip, leaving only the centre of the bell uninjured, stimuli applied to the extreme end of the strip passed along it, and were delivered to the centre of the bell, just as if they had been applied to the central part itself—all passing at the same rate they did not interfere with one another. But when the strip was pressed upon or stretched, the passage of impulses was interfered with.

This seems to show that the rate of transmission of a stimulus along a conducting structure is a definite one, provided the structure remain under the same conditions. But still more instructive on this point are the experiments with the Ton-inductorium, invented by my friend Prof. Hugo Kronecker. Other observers have found that when a muscle is irritated by an interrupted current applied to its nerve, the tetanic contraction into which it would be thrown by twenty interruptions per second ceased when the interruptions became as frequent as 250 per second. By using an interrupted current induced by the vibrations of a magnetic rod, which gave out a definite tone, Kronecker and Stirling were able to throw the muscle into tetanus with no less than 22,000 interruptions per second. This success is probably to be attributed to the regularity and equality of the stimuli applied by Kronecker's method, while the fact that their predecessors got no tetanus with more than 250 interruptions per second is probably due to interference of the stimuli they applied.[2] Kronecker's observations show, I think, how definite must be the rate of transmission of stimuli along a nerve so long as it remains under the same conditions and give us a basis for extending the theory of interference from waves of light and sound to vibrations in nervous and muscular tissues.[3]

We are justified, I think, by these experiments in considering that interference may occur in the nervous system, and that one part may exercise an interfering or inhibitory effect upon the other, which is constant under normal conditions, but will be modified when these conditions are altered.

Let us now try to apply this hypothesis to the reflex action which we have just been discussing.

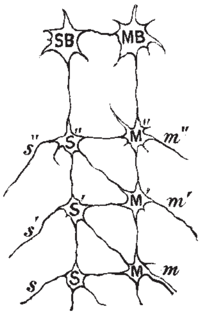

FIG. 5.

Let S, S′ and S″ be three sensory cells in the spinal cord, M, M′ and M″ motor cells, s and s' sensory nerves, and m m' motor nerves. S B is a sensory and M B a motor cell in the brain. When *s* is stimulated by a slight touch, the stimulus is transmitted up to S, thence to M, and down *m* to muscles, thus causing reflex contraction. This is increased when a number of slight touches are made over a limited surface, as in tickling, because then S and S′ are both stimulated, and more motor impulses are produced. But when harder pressure is made on *s* the stimulus, instead of being confined to S, is transmitted to S′ and thence to M, as well as direct from *s* to M. Thus two impulses are sent to M, which, starting at the same time from *s*, have had a different length to travel round. This different length, we suppose, is just sufficient to allow the impulses to interfere with one another in M and thus destroy each other's action in regard to motion. When *s*′ is also irritated at the same time as *s*, the same interference is produced by a stimulus passing from S′ to S″, and then to M′. But at the same time that the relation of S′ S″ to M and M′ is such as to produce interference and inhibition in regard to motor impulses, the relation to each other is such that the impulses mutually strengthen one another on their way up to the brain, and thus the sensation which we perceive on firm pressure is more definite and better localised.

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[1] Archiv. f. Anat. u. Physiol. 1860, p. 798.

[2] It must be borne in mind, however, that the overtones of such a vibrating rod are in the ratio of n, $3n$, $5n$, &c., and not in that of n, $2n$, $4n$, like those of a vibrating string or pipe. Quincke (Poggendorff's Annalen, 1866, vol. viii. p. 182) failed to silence the sounds of such a rod by means of an interference apparatus.

[3] *Vide* Hermann's Handbuch d. Physiol. Bd. i. Th. i. p. 44, and Bd. ii. Th. i. p. 39.

On this hypothesis each successive layer of sensory and motor cells in the spinal cord may have several different functions: (1) Each cell may exercise its own sensory or motor functions in relation to the sensory or motor nerves connected with it; (2) it may exercise an inhibitory function on the sensory and motor cells above or below it, and also on other sensory or motor cells on the same plane with itself; (3) it may have a stimulating function on other cells above, below, or on the same plane as itself, increasing instead of abolishing their action.

The effect that any sensory or motor cell produces when stimulated is not determined then simply by the *properties* of the cell itself, but by its *relations* to other cells or fibres.

Motion, sensation, inhibition, or stimulation are not positive, but simply relative terms, and stimulating or inhibitory functions may be exercised by the same cell according to the relation which subsists between the wave-lengths of the impulses travelling to or from it, the distance over which they travel, and the rapidity with which they are propagated.

T. LAUDER BRUNTON

(*To be continued.*)

NOTES

M. JANSSEN was present at the sitting of the Academy of Sciences on Monday, for the last time before his departure from Paris. He is very busy preparing his apparatus.

BARON NORDENSKJÖLD so very carefully considers every step he takes that we may be sure he has satisfactory reasons for claiming the reward of 25,000 guilders (about 2000*l.*) offered by the Dutch three centuries ago to the discoverer of the North-east Passage. Some surprise is expressed at the Baron's claiming a reward which lapse of time may be considered as having rendered obsolete. At the time it was offered the North-east Passage was regarded as a sea-route of the highest commercial importance, though this idea has been long exploded. Still to some extent Baron Nordenskjöld has shown that the old conception was not without justification, and although the passage is now of no value as a route to China and India, still the Swedish explorer has proved that as a trade-route it may be rendered of considerable value. Moreover as he is so disinterested, ardent, and successful a pioneer of science, we should be glad if the Dutch Government cheerfully admitted the claim. It may be remembered that the much larger reward offered by our own Government to the discoverer of the North Pole was withdrawn many years ago.

AT the last meeting of the Royal Swedish Geographical Society, on the proposal of Baron Nordenskjöld, the greatest honour at the disposal of the Society, the *Vega* gold medal, was conferred on Mr. Stanley. The medal, struck *in memoriam* of the *Vega* expedition "for geographical discovery," has only been twice before conferred—viz. in 1881, on Baron Nordenskjöld, and in 1882, on Capt. Palander.

IN 1880 the Belgian Academy proposed, as a prize-subject, the relations between physical and chemical properties of simple and compound bodies (completion of the knowledge of these by new experiments). The prize (a gold medal valued at 1000 francs) has been awarded to M. De Heen, engineer at Louvain. His memoir is an extension of one previously sent in, which gained high approbation for original work and results, but was thought badly-proportioned, so that the subject was re-proposed. The work is in five sections, dealing successively with specific heats, dilatability of solids and liquids by heat, changes of state in relation to chemical composition, capillarity, and (here without original researches) molecular volumes, refraction, spectral analysis, and absorbent power of bodies for heat. The ample *résumé* M. Spring gives of this memoir (*Bull. Belg. Acad.* No. 12) indicates matter that must be of much interest and value to the physicist and the chemist.

PERHAPS never in the history of science, the *Lancet* says, has a distinguished career equalled in its length that of M. Chevreul, whose name is best known in this country in connection with his investigations on colour; and it is probably altogether unique for a *savant* to be able, at one of the most distinguished scientific societies in the world, to refer to remarks which he made before the same society more than seventy years previously. A few days ago M. Chevreul made a communication to the Académie des Sciences, and at its close he observed: "Moreover, gentlemen, the observation is not a new one to me. I had the honour to mention it here, at the meeting of the Académie des Sciences, on the 10th of May, 1812"!

THE death is announced of the Silesian botanist, Herr Johann Spatzier, aged seventy-seven; also of Herr Josef Knörlein, the entomologist, at Linz, on February 12, aged seventy-seven.

MOUNT ETNA is very active and ejects red-hot lava. At night the glare is constantly visible. A violent shock occurred on February 15.

WE find in the last number of the *Izvestia* of the Russian Geographical Society a note, by Prof. Lenz, on the cosmical dust collected by M. Marx at the meteorological station of Yeniseisk. After having vainly searched for traces of cosmical matter, as he was advised to do by Baron Nordenskjöld, he discovered it finally on October 31, 1881. The wind was blowing in the evening with great force from the west, and during the night it turned into a strong gale, with some snow and rain. When M. Marx measured next morning the amount of water in his pluviometer, he remarked that it had a considerable quantity of suspended matter of a brick-red colour. After careful analysis this matter proved to consist of iron, nickel, and cobalt. Prof. Lenz does not doubt that the red dust found by M. Marx had a cosmical origin, and points out that it was observed on a day very near to the appearance of the November meteors.

AT Monday's meeting of the Paris Academy of Sciences, M. Tresca read a paper full of facts on the experiments tried at the Gare du Nord. Deducting certain work for the mechanical transmission to the generator, the result was 42 per cent. of energy conveyed instead of 35 per cent. with a smaller